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A Reply to Mr. Christie's Paper "On Mr. Stone's Alterations of Bessel's Refractions." By E. J. Stone, Esq.

In the *Monthly Notices* of the Society, vol. xli. No. 3, January 1881, there appears a paper by one of our Secretaries, Mr. Christie, which renders a reply desirable.

I am of opinion that good, and not harm, results from the full discussion of differences of opinion on scientific points; but the facts must not be obviously strained on either side. I should not have considered it necessary to reply to Mr. Christie were it not that some statements, on page 95, have been seriously misunderstood by many readers; and that some others, on page 107, appear to me to be deficient in precision.

In my paper on Mean Refractions, vol. xl., No. 6, 1880 April, I brought forward—at the request of some Fellows of the Society—the result of a recomputation of the residual error for the group of stars, 43° to 46° 29′, which was originally given in my paper on Bessel's Mean Refractions in the *Monthly Notices* for November 1867. The following statements are extracted from my paper of April 1880, pp. 338 and 339:—

"It may probably avoid useless discussion if I state clearly all the corrections applied to the Greenwich results.

"First: The observations have been extracted from the Annual Catalogues of the Greenwich Observatory 1857 to 1865 inclusive.

"Secondly: I have not used the Reflexion Observations. These observations have been already combined with the direct observations in the determination of the Nadir Points and in the deduction of the R—D corrections, and the corrections thus deduced have been applied in the Greenwich volumes to the direct results. With this exception, all the results given in the Greenwich volumes have been used.

"Thirdly: I have assumed that the thermometer used in 1857 and part of 1858 read too high by six-tenths of a degree Fahrenheit.

"Fourthly: I have assumed that the barometer used in the reductions read too low by a hundredth of an inch.

"Fifthly: As some of the observations contained in the group (5) have a greater zenith distance than 82°, and these observations have had their refractions already decreased by 0·0023 × tabular refraction, I have therefore increased their refractions by the same amount to bring them back to the results which would have been obtained had Bessel's Refractions been used unaltered.

"Sixthly: The observations have all been reduced to an assumed colatitude 38° 31′ 21″·80.

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"Seventhly: I have weighted the observations from the formula—

$$W = \frac{1}{2\epsilon^2 + \frac{e^2}{n} + \frac{e'^2}{n_{,}}} \quad .$$

assuming $2\epsilon^2 = 0{\cdot}04$, and extracting e and e' from my paper on Probable Errors in the *Monthly Notices* for 1869. The result obtained in 1867 was 2″·19 with a weight 28, or probable error ± 0″·2. The result which I now obtain is 2″·10 with a weight 24, or probable error of ± 0″·2. The two results are essentially identical, and it is clear that all the corrections now applied were applied in 1867."

I am unable to appreciate the grounds upon which Mr. Christie can ever have supposed that, in a discussion regarding the necessity for certain breaks of continuity in Bessel's Refraction adopted at Greenwich, I carried on that investigation without taking these breaks of continuity into consideration. But I certainly thought that the statements contained in my paper of April 1880 were sufficiently explicit. It appears, however, that I was mistaken, for on pp. 95 and 96 of Mr. Christie's paper in the *Notices* for January 1881, we may read as follows—

"Notwithstanding Mr. Stone's explicit statement in the *Monthly Notices* for April 1880, pp. 338, 339, of the corrections which he has applied in forming his last residual, there are several points to be cleared up—

"1. Corrections for error of thermometer and barometer are stated to have been applied to the observations in 1857 and part of 1858. Now, I do not find in the Greenwich 'Introductions' any observations in those two years which would fall in this group.

"2. The limits of Mr. Stone's last group are N.P.D. 43° to 46° 29′. I have not been able to find any observations in the Greenwich 'Introductions' between N.P.D. 45½° and 47½°.

"3. The assumed co-latitude is stated to be 38° 31′ 21″·80, but in his paper of 1867 Mr. Stone, after applying a correction −0″·205, obtains 38° 31′ 21″·61, which would imply an assumed co-latitude 38° 31′ 21″·815. The difference is trifling, but it must be remembered that Mr. Stone gives his residuals to a thousandth of a second.

"I call attention to (1) and (2), not in any spirit of criticism, but because there seems a possibility that as Mr. Stone extracted his results from the Greenwich Annual Catalogues, his last group may contain observations accidentally omitted from the Latitude Investigations in the Greenwich 'Introductions' which I used.†"

[Note, page 95: "† On examining the Greenwich Observations for 1857, I find that there are, as a matter of fact, several stars in the Annual Catalogue which, for some reason, are not included in the Latitude Investigation in the Introduction. Mr. Stone had not stated in his paper of 1867 that he had taken his

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results from the Annual Catalogues, and I thought it more natural that he should have taken them from the Introductions where they are collected. Thus our data are not the same."]

But in continuation we find, page 96—

"As regards the residuals, there is a point of some importance on which I am at direct issue with Mr. Stone—viz. the estimated probable error of the result for the last group. Mr. Stone obtains a weight of 28 or 24 with a probable error $0''\cdot2$, while with the same system of weights I obtain a weight 6·5, with a probable error of $0''\cdot4$. I have for the last group taken the probable error of an observation above the pole as $0''\cdot5$ and S.P. as $1''\cdot5$, and I have altogether 30 observations above pole and 17 below, which gives as an *inferior* limit to the probable error $0''\cdot38$, on the supposition that they all referred to the same star. It is just possible that the additional observations included by Mr. Stone may account for the discrepancy.

"In any case there are, I believe, but few stars observed in this group; and where the same star—*Capella*, for instance—is observed every year on the same division of the circle, account ought to be taken of the probability of a systematic error affecting the series. Thus the weight ought not to be assumed to be the sum of the weights for the separate years. Unless Mr. Stone has followed this last and erroneous system, I hardly see how he can have got such a large weight and small probable error for the result of his group."

I do not profess to understand the real drift of these statements of Mr. Christie. But there is an uncomfortable look about the combination of this pointed reference to pp. 338, 339, of my paper in the *Notices* for April 1880, and Mr. Christie's paragraphs (1) and (2), which has misled many readers, and conveyed an impression which I am sure that Mr. Christie could never have intended.

It is clear, from the note given at the bottom of page 95, that Mr. Christie is quite aware that he has overlooked a number of Greenwich observations which have an important bearing on these questions of refraction. As a matter of fact, there are in the Greenwich volume for 1857 no less than seven different stars observed, which fall within the group N.P.D. 43°—46° 29′, with 30 observations above the pole and 28 below the pole, whose results are not given in the Latitude Investigation in the Introduction. It appears now that these observations, and some others, have been omitted by Mr. Christie from his discussions. It is clear, therefore, that all force is removed from the paragraphs (1) and (2), which prove no error or misstatement whatever on my part; and it is equally clear that Mr. Christie was not likely to obtain as small a probable error for the group as that obtained by me, because he had not made use of *one half of the materials available for the discussion.*

I greatly regret that Mr. Christie has thought it necessary to publish criticisms which appear to me to derive whatever force

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they may have, in the minds of his readers, from misconceptions of his meaning and intention.

I now come to the consideration of the statements made by Mr. Christie on page 97.

Mr. Christie considers my statement, "that the accumulated evidence of the Greenwich circumpolar observations is against the accuracy of Bessel's Refractions unaltered," as "really quite misleading." If I thought that such was the case the statement should be at once withdrawn; but the statement appears to me perfectly correct after the best reconsideration which I can give it. In 1857 Mr. Main discussed the Greenwich observations (below 70° zenith distance) made during the years 1836 to 1854, both inclusive. The practical result of that discussion was that Bessel's Refractions were abandoned at, and below, 82° Z.D. and another set of Tables = Bessel's Tables (1 – 0·0023) used for zenith distances between 82° and 85°. At 85° this second set of Tables was replaced by a third = Bessel's (1 – 0·0049). I presume, therefore, that the Greenwich observations 1836 to 1854 are not represented within probable errors by Bessel's Refractions unaltered, or these large and arbitrary changes would not have been considered necessary in the reduction of the Greenwich observations.

My investigation, made in 1867, discusses the observations made at Greenwich 1857 to 1865. The residual errors for five groups with N.P.D—0° to 15°; 15° to 30°; 30° to 37° 40′; 37° 40′ to 43°; and 43° to 46° 29′—are as follows:—

	(1)	(2)	(3)	(4)	(5)
With Bessel's Refractions, A	+0″·240	+0″·196	+0″·570	+1″·054	+2″·190
With Bessel's (1–0·0053) R. B	+0·15	–0·10	–0·14	–0·08	+0·10
The weights are	130	88	31	54	28

From 1868 to 1876 the Greenwich observations were reduced with Bessel's Refr. (1 – 0·0053), and the residual errors, with the colatitude determined, as it should be, from the observations themselves, but without attributing any weight whatever to the two last groups, which are chiefly affected by errors in the adopted refractions, are as follows:—

	(1)	(2)	(3)	(4)	(5)
C	+0″·15	–0″·22	+0″·05	–0″·31	+0″·22.

In this case the last two groups contain observations to 85° and below 85° respectively. Now, I have no hesitation in saying, no practical astronomer with the residual errors B and C before him, and informed that they contain the excess of N.P.D. below pole over N.P.D. above pole, for groups extending, in the case

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of B to 85° Z.D., and in the case of C to below 85° Z.D., would, if in ignorance of what refraction tables had been used, express any other opinion than that the agreement was remarkably close, and proved the substantial accuracy of the mean refractions adopted. These are the facts upon which my statement was based, and I cannot recall it. But Mr. Christie refers to his paper (*Memoirs*, vol. xlv. p. 161) as affording some evidence against these diminished refractions and in favour of Bessel's Refractions unaltered. Let us see what this evidence amounts to. Of the four periods discussed by Mr. Christie, the observations made 1840–1847 are not extended below zenith distance 78°, and in such a discussion Mr. Christie was hardly likely to obtain any positive evidence against the refractions adopted for the determination of the colatitude. With respect to the periods 1851–1861, 1862–1865, Mr. Christie has unfortunately grouped together observations reduced with Bessel's Refractions and observations reduced with refractions obtained by throwing away from Bessel's Refractions quantities varying from 0''·9 to over 2''. And it now appears that he has omitted from the discussion altogether a large portion of the most valuable material, valuable so far as questions of refraction are concerned, which is available in the Greenwich volumes. I consider, therefore, that Mr. Christie's work requires revision throughout.

And here I must call attention to what, after giving the matter a careful consideration, appears to me a serious fallacy which invalidates Mr. Christie's work upon these questions of refractions. The refractions which he uses for comparison with the results obtained, 1868 to 1876, are not in reality Bessel's Refractions. They are Bessel's Refractions unaltered down to 82° zenith distance. At that zenith distance the error of Bessel's Refractions has apparently become sufficiently large to be distinctly put in evidence, and to be separated from the systematic instrumental errors to which all astronomical results are liable. At 82° a quantity of about 0''·9 is thrown away from Bessel's Refractions, and a new set of Tables used from 82° to 85° Z.D. At 85° Z.D. the error has apparently again become sufficiently large to be put in evidence, and the refractions are at 85° Z.D. a second time arbitrarily diminished by about 2''·9 from Bessel's Refractions, and a third set of tables used below 85° Z.D. Need I point out that the more conclusively it can be shown that the observations are represented by refractions thus computed, the more certain it is that *Bessel's Refractions unaltered* do not and cannot represent the Greenwich observations.

I know nothing about the errors of Bessel's Refractions except what I can gather from the observations. If these large corrections and abrupt breaks at 82° and 85° are not required, they should not be applied; but if they are required, it is useless to contend that Bessel's Refractions unaltered represent the Greenwich observations. And surely it is unnecessary for me to point out, if Bessel's Refractions are too great by 0''·9

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at 82° Z.D. and the error is increasing in the same direction to 85° Z.D., that Bessel's Refractions cannot be without sensible error at zenith distances less than 82°. The error cannot change *per saltum* at 82°.

If the errors of Bessel's Refractions are such quantities as those given in my paper of April 1880, viz. +0″·4 at 70°, 0″·16 at 76°, 0″·28 at 78°, they are hardly likely to be detected from a general discussion of observations limited to an arc of 78°, when the observations are affected, as all the observations with which I am acquainted are affected, with systematic instrumental errors of at least equal or larger amounts.

With respect to what Mr. Christie, on page 104, calls my "*tinkering*" of Bessel's theory. What I have done is as follows:—Bessel's theory has two constants to be determined from observation. He first tried to include the refractions from the zenith to the horizon under one general investigation, and the errors of his theory were not, as a matter of fact, very great. But it was clear to Bessel that his refractions were too large at 85° by about 2″; and he recommended that for greater zenith distances than 85° the results of his theory should be replaced by refractions deduced more directly from observation. Now in the *Notices* for April 1880, I pointed out, and I think that any one who has had much experience in such investigations will recognise the importance of the point, that when Bessel found it necessary to abandon his attempt to include all the refractions from the zenith to the horizon in one investigation, and restricted the theory to a smaller arc than 90°, he should have re-determined both the constants of his theory from observations included within the restricted arc, and not allowed one of the constants to be strained by attempts to represent observations near the horizon, attempts which had confessedly not been successful.

What I think Bessel should have done when he found his refractions too large at 85° Z.D., I have done, and the results are given in the *Notices* for April 1880. I have, as there clearly stated, assumed the error at 85° Z.D.=2″·87. This quantity may be, for aught I know, too large from the accidental run of the systematic errors of the Greenwich observations. But I have no good reasons for supposing such to be the case. If the assumed error be sensibly correct, then the corrections given by me for Bessel's Refractions at smaller zenith distances are entitled to be received with confidence; but if the error 2″·87 is much too large for Z.D. 85°, then the errors given for the smaller zenith distances will also be slightly too large. If this be *astronomical "tinkering,"* all I can say is, that I am well content to be an "Astronomical Tinker."

With respect to Mr. Christie's objections to my use of the co-latitude determined from the observations 1868–1876 in exhibiting the residual errors of these observations, I am quite unable to follow Mr. Christie's reasoning. If the method, adopted at Greenwich 1871–1875, of applying a correction of 0″·6 to

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0″·8 to the nadir determinations before using them, in combination with the reflexion observations, for the determination of zenith points was perfectly satisfactory, then the co-latitudes determined from the observations should be perfectly satisfactory, and should be used, as I have done, and not rejected, as Mr. Christie has done. If this method of treating the not inconsiderable discordance between the nadir points, although the best practical method of meeting a confessed difficulty, is not altogether satisfactory, then the use of the co-latitudes determined from the observations themselves should, to a great extent at all events, correct any outstanding constant error; but, to secure this, the co-latitude deduced from the observations which are affected with the same constant error must be used. Mr. Christie's argument, on page 108, although brought forward to prove me wrong, affords, in my judgment, the most satisfactory evidence that could be adduced in favour of the course which I have adopted. "The apparent change in the co-latitude on which Mr. Stone remarks has nothing to do with this correction (*i.e.* constant difference between the nadir points with reflecting eyepiece and reflexion stars), but is simply due to a change in the system of weights given to the close circumpolar stars and to the increased number of observations of wide circumpolars. It did not begin in 1871, when the correction to the nadir observation was first applied, but in 1872, when special attention was directed to the observation of wide circumpolars. Previous to that year the large weight given to the four close circumpolars made the value of the co-latitude virtually depend on them; and if this system had been continued the same value would have been obtained in subsequent years. The four close circumpolars give for the period 1868–1876, 38° 31′ 21″·95 with Bessel's Refractions, and 38° 31′ 21″·70 with the diminished refractions; whereas the stars about N.P.D. 20° give about 38° 31′ 21″·50 and 38° 31′ 21″·17. It is simply the admission of these latter stars to a fair share in the result which has produced an apparent change. The apparent change does not justify Mr. Stone in using two different co-latitudes in the periods 1857–1865, and 1868–1876." Surely, if the smaller co-latitude given by these observations, 1868–1876, is due solely to a better distribution of the weights than that formerly adopted, Mr. Stone is, at all events, justified in using the co-latitude deduced from these observations, and Mr. Christie is certainly not justified in rejecting it in favour of a value which would have been obtained with a more imperfect system of weights. But we are really concerned in these questions with the effective co-latitude (*i.e.* the actual differences of readings on the circles between the mean adopted nadir point, or zenith point, and the polar point) and not with the real colatitude of the Greenwich Observatory, even should there, as a matter of fact, be any sensible difference between them.

I must now refer to some statements of fact by Mr. Christie,

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on page 107, which I do not think are made with the precision which is desirable.

"In 1868, at Mr. Stone's suggestion I believe, the practice was commenced of taking the nadir point at 0^{r} of the microscope micrometers, and thus was obtained that accordance of separate results which is so dear to some astronomers, but which is not always conducive to the elimination of systematic errors. The additional wear resulting from this increased use of one part of the screws gave rise to a discordance between the nadir observations and observations of stars which were taken at all parts of the screws. This discordance is shown in the years 1869 and 1870 by the large constant term in R – D; in subsequent years a correction was applied to each nadir observation derived from the mean results for the preceding year. In 1875, being dissatisfied with the large correction for runs virtually required to reduce the nadir observation to the mean of observations of stars which would fall at about $2^{r}\cdot 5$, I suggested that the nadir observation should be taken at from $2^{r}\cdot 5$ to $3^{r}\cdot 5$, and it was then found that the correction to the nadir observation had changed from $-0''\cdot 8$ to $+0''\cdot 6$. This threw suspicion on the micrometer screws, and their errors were examined, as explained in my paper of 1876. The result was to show that corrections were required ranging from $-1''\cdot 4$ at $\bar{1}^{r}\cdot 3$ to $+0''\cdot 8$ at $2^{r}\cdot 3$, the correction at $0^{r}\cdot 0$ being $-0''\cdot 9$, corresponding very closely with the correction to the nadir observation $-0''\cdot 8$. Further, observations taken for another purpose in 1868 showed that the errors of the micrometer screws were sensibly the same at that time, with the exception of the portion about $0^{r}\cdot 0$, which had suffered extra wear since 1868 from a preponderance of some 4,000 observations taken at this part."

If this statement means that in 1868 the practice was introduced of taking the nadir points near the readings $0^{r}\cdot 0$ of microscope A, then it is certainly not correct. If it simply means that, to avoid very large corrections for run, the nadir points were generally taken, after 1868 Feb. 1, at readings between $0^{r}\cdot 0$ and $1^{r}\cdot 5$, or over a range of the screws with which more than a quarter of all the observations made with the circle would be made, then the statement may be accepted as substantially accurate. But, in this case, it will fail to account for the serious wear of the screws near $0^{r}\cdot 0$, which Mr. Christie supposes to have taken place shortly after the introduction of the change. I have taken the trouble to examine the Greenwich observations 1868 and 1869, and I find as follows. From 1868 Feb. 1 to 1869 Dec. 31 there were only 108 determinations of the nadir point at readings of microscope A less than $0^{r}\cdot 2$, whilst there were 72 determinations made at readings of microscope A above $1^{r}\cdot 0$. The largest reading is $3^{r}\cdot 0$, but there are several as large as 2^{r}. Whatever, therefore, may have been the cause of the state of the screws near $0^{r}\cdot 0$ in 1876, it certainly was not caused by the additional 4,000 observations made near $0^{r}\cdot 0$ at my suggestion.

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I left for the Cape in 1870. The last discussion of the R − D which I could have had the advantage of seeing was that for 1868. But if the screws were worn, in the way indicated by Mr. Christie in his paper of 1876 November, so that the error changed from $-1''\cdot 2$ at $-1^{r}\cdot 9$ to $-0''\cdot 9$ at $0^{r}\cdot 0$ and to $-0''\cdot 5$ at $0^{r}\cdot 1$, then the values of the *runs* determined for readings somewhere near $0^{r}\cdot 0$ to $4^{r}\cdot 8$ or $4^{r}\cdot 9$ must have been seriously affected, and have depended very much indeed upon the exact reading from which the run was actually commenced. From the form of the curve of errors given in Mr. Christie's paper 1876, I should have supposed that the runs had not been corrected for errors of screws before their application, and that this oversight had caused the errors of the screws to be *apparently* nearly the same for the large readings near $4^{r}\cdot 8$ and for the small readings, from which the run was generally started; but I am persuaded that such an oversight could not possibly have been made by Mr. Christie in a work of importance.

The correction of the Greenwich Results for these assumed errors of screws cannot be completed without a correction of the runs, of the readings of the screws employed in making any particular observation, and of the readings included in the adopted zenith point determination; and as the actual readings from which the runs are determined are not published, I have been unable to correct the Greenwich Results for errors of screws. But fortunately such corrections are certain, in mean results, to be very small; and in questions of errors of our refraction tables at such zenith distances as 82° to 85° and 85° to 87° with which I was concerned, such errors are altogether unimportant.

As discussions involving in any degree personal questions have a tendency to obscure the real points at issue, and as I have neither the time nor inclination for mere verbal discussions, I will state, as clearly as I can, what I maintain and what I admit with respect to these questions of refraction.

1. I maintain that the Greenwich circumpolar observations are not represented within errors of observation by *Bessel's Refractions unaltered* from 0° to 85° Z.D. Bessel himself recommended their abandonment below 85° Z.D.

2. I maintain that Bessel's Refractions $(1-0\cdot 005)$ do, whether accidentally or not I cannot say, represent, within errors of observation, the Greenwich circumpolar observations 1857 to 1876.

The co-latitude must of course be determined with the same set of refraction tables.

3. I infer, from the general results of Main's discussion of the observations 1836–1854, that if these observations be treated *as a whole*, and no abrupt breaks in continuity of the refractions used be admitted, then they would be represented either within, or very nearly within, errors of observation, by the use of Bessel's Refractions $(1-0\cdot 005)$. This I think is shown in my paper of April 1880.

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4. I point out that the observations 1836–1854 and the observations made 1857 to 1876 at greater zenith distances than 85° are closely represented also by these diminished refractions.

5. I freely admit that a comparison between the Cape observations, which I have recently put into the hands of astronomers, exhibits a most astonishingly close agreement, for moderate zenith distances, with the Greenwich Results when the observations are reduced with Bessel's Refractions unaltered; but the observations show that Bessel's Refractions at 82°, and certainly at 85°, are much too large for the Greenwich observations.

6. I maintain that any refraction tables, to merit acceptance, should satisfy the known conditions without any abrupt breaks of continuity over the arc for which they are used.

7. I fully admit that the solution obtained by me in 1867, which satisfied the then known conditions, does not satisfy *all* the conditions brought forward by my Cape work.

8. I maintain that Bessel's Refractions unaltered fail to represent either of the necessary conditions to 85° Z.D.

9. I believe that by a slight but necessary correction of Bessel's theory, brought forward in the *Notices* for April 1880, the conditions, so far as I know them at present, are fulfilled.

I therefore, of course, much prefer the solution of 1880 April to that of 1867 November. The one satisfies one set of conditions, whilst the other satisfies two. A complete solution must satisfy all the conditions of a problem. A solution to be worth acceptance even for a time, and until replaced by something better, should satisfy all the known conditions. But, if it does satisfy the known conditions, it is worth acceptance until facts inconsistent with it have been distinctly put in evidence. It must then, of course, be modified or abandoned. I know of no method by which progress, and not stagnation, can be secured, except by thus testing and modifying our theories as fresh data are brought forward.

What I have objected to, and do most strongly object to, is that directly some fresh conditions for testing these questions of refraction were brought forward by my publication of the Cape Catalogue 1860, some astronomers appeared to rush to the conclusion that these conditions were alone worth consideration, and that all our previous work was wrong; and (overlooking, or at least not giving any sufficient prominence to, the fact that the Greenwich observations, which agreed so well with the Cape observations, agreed at considerable zenith distances only by a virtual abandonment of Bessel's Refractions) they upheld the accuracy of Bessel's Refractions unaltered, and attempted to explain away the force of the evidence slowly accumulated at Greenwich by the labour of years; and, whether intentionally or not, certainly created a false impression that I had committed some serious mistake in my paper of 1867. No mistake, so far as I am aware of, was made; the whole of the available data were represented by the solution obtained.